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Patterns and Prescriptions in Mexican Historiography

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This article offers a short resumé of recent Mexican historiography in the national (post-1810) period, noting three clusters of innovative research: post-independence politics; Porfirian economic history; and regional studies of the Mexican Revolution. It then addresses the recent call for historians of Mexico and Latin America to 'reclaim the political', analysing the implications of this kind of bold prescription which, it argues, is misguided in both historiographical and political terms.

Keywords: Mexican history, Mexican historiography, theories of history.

'Instead of writing history, we are always beating our brains to discover how history ought to be written' (Hegel).

The invitation to present an overview of Mexican historiography: where it was going and – perhaps – where it should go, was an interesting but risky proposition.[1] Lord Byron, as Lady Caroline Lamb famously put it, was 'mad, bad and dangerous to know'; and writing historiographical surveys of this kind has a certain Byronic quality, since historians who take on such tasks run the risk of seeming mad, bad, or dangerous to know. Mad, because, unless the task is narrowly and carefully defined, it may be insuperable and therefore ill-advised, even mildly crackpot; bad, because, unless the historian opts for a deliberately bland, boring and consensual approach, he may well give offence; and, for the same reason, dangerous to know and, very likely therefore, the target for future reprisals. I base these generalisations in part on personal experience.[2] However, if one opts to take up the challenge, it seems both feeble and boring to aim for bland consensus.

The task is often insuperable because, as I shall later argue, the output of history has become so extensive that no individual can reasonably claim to monitor production or achieve genuine quality control. The widgets are flooding out of the factory and, at

1 This paper was originally presented at the January 2005 meeting of the Conference on Latin American History, which meets as part of the American Historical Association.

2 Not least a bruising encounter at Oaxtepec, at the VI Conference of US and Mexican Historians in 1989, where my attempt at a *tour d'horizon* of Mexican revolutionary history received a rather brusque reception.

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best, we watch the flow and hazard informed guesses about their number and whether the mark IV widget is an improvement on its mark III predecessor. (Warning: this paper is uncharacteristically full of mechanical, especially automobilistic, tropes, though the latter are offset by some astronomical ones too). Even if the field of inquiry is carefully limited – in this case, to Mexican history – it remains extensive and plenty large enough to get lost in. I did therefore make a half-hearted effort to quantify output: to list the books and articles published in recent years and to apply some sort of conventional typology (by period: colonial/national; or by approach: political, social, economic, cultural). But – lacking the services of dedicated research assistants – I soon gave up. As Peter Novick ruefully recalls in his masterly book *That Noble Dream*:

> some time ago I spent the better part of two years coding the evaluative language used in thousands of historians' book reviews, punching IBM cards, and attempting to correlate the language employed with dozens of other variables having to do with historians' generation, field, status, etc. It was a total waste of time, producing nothing intelligible and permanently dampening my enthusiasm for introducing quantitative rigor into intellectual history (1988: 8, fn. 7).

Furthermore, the conventional categories present difficulties. Periods, which, especially in the case of Mexico, have a certain mathematical precision (as we know, all Mexican history *in tres partes divisa est*: pre-1519, 1519–1821, and post 1821) are in fact open to serious question, especially given the current vogue for the 'middle period', which straddles the 1821 watershed (Szuchman, 1989; Voss, 2002: xi–xiii; cf. Taylor, 1985: 122–123). Even more obviously, thematic categories are open to question: some of the new cultural historians would consider themselves to be resolutely political (Vaughan, 1999); while some old-timers might query whether the new cultural history is not the old social history writ differently and often writ worse.[3] So, the 'new cultural' history may be neither so new nor so cultural as its protagonists – or indeed its critics – would have us believe. A short digression: I do not want to turn this paper into yet another discussion of the new cultural history (NCH), since, for one thing, it would be tediously repetitive. But like the elephant in the living room, it can hardly be ignored. In this context, the problem – which remains, as far as I can see, unresolved and often unspoken – is the definition of 'cultural', which supposedly demarcates this brand of history from 'social' history or any other brand. In most cases, the meaning of 'culture' or 'cultural' is taken for granted; it becomes another of those 'self-evident truths' which litter the roadside of intellectual history.[4] Again, this is not

3 It seems to me that several of the supposed characteristics of the 'new cultural history' – a concern for subaltern agency, for 'process', for political engagement and, of course, for 'cultural' explanations – are all present in E. P. Thompson's *The Making of the English Working Class* (1968). In part for these reasons it was also seen as 'seriously flawed' by some critics: Fischer (1970: 85).

4 Knight (2002). Adequate definitions/discussions of 'culture' (in the context of the 'new cultural history') are few and far between. Appleby, Hunt and Jacob (1995: 120, 154, 218–223), *Telling the Truth about History*, flatters to deceive; Joseph, Rubinstein and Zolov (2001: 16), 'Assessing the Fragments: Writing a Cultural History of Mexico since 1940', hardly bothers. Burke (1992: 118–126), *History and Social Theory*, is one of the better efforts.

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a new problem, since debates over the demarcation of historiographical categories are old (Evans, 1997: Ch. 1). It is not even an important problem, so long as we are engaged in doing what historians usually do, that is, researching and writing history – what, in conclusion I refer to as changing the spark plugs. It matters only when we are engaged in more rarefied historiographical and quasi-philosophical lucubrations – that is, designing cars powered by nuclear fusion. Fortunately, we don't have to do this too often or even, I suggest, too seriously.

Thus, when we attempt broad evaluations of output, we are not in fact counting widgets – discreet objects of a certain weight, shape and value – but rather a host of incommensurate products, large and small, valuable and worthless; some elegant, some ugly, some genuinely new and useful, some merely re-bored widgets, churned out (in the United Kingdom at least) to meet the latest Stalinist targets of the RAE ('Research Assessment Exercise').[5] In fact, rather than counting widgets, evaluating history is more like trying to census and categorise the 350,000 known species of beetle which inhabit the earth. (Which of our colleagues corresponds to the dung-beetle?).

Thus, apart from measuring the sheer volume of production, we also need certain qualitative criteria by which to gauge the value of what is produced. (We have all heard of 'seminal' articles – although I suspect we have in fact read rather few – but it would be hard to say what defines 'seminality'). I think three principal criteria determine historiographical quality: being *right*; being *clear*; and being *original*.[6] Let me say a word about these three criteria, with occasional reference to some Mexican sources by way of illustration.

(i) Being *right* means achieving some reasonable and demonstrable representation of what happened. By making this statement I assume that this is a valid and feasible exercise and that historians are not engaged in merely concocting and swapping texts which bear no such relation to what happened. (Critics of the NCH have alleged that this is what goes on – that 'anything goes'[7] – but I think their criticism, however understandable in light of some NCH failings, is probably exaggerated, at least in most instances). Two rather elementary errors are often made when discussing this, the old question of historical 'objectivity'.[8] The

5 The RAE is a device whereby the British government purports to evaluate the quantity and quality of research done by university departments, adjusting the flow of resources accordingly. Following the law of unintended – but thoroughly predictable – consequences, research snowballs, especially as the RAE deadline approaches; journals are swamped with submissions; universities poach productive academics ahead of the transfer deadline; teaching suffers; quantity trumps quality (since it is more easily measured); and short-termism prevails.

6 Here I develop an argument previously made in Knight (2004).

7 Haber (1999). In fact, despite a good deal of waffling and water-muddying, the overwhelming majority of historians – in this case, of Mexico – do not go as far as Haber fears; those who make a 'perversely gleeful denial of the possibility of historical knowledge' are mercifully few and far between. The phrase is from Emilia Viotti da Costa (2001: 24).

8 Citations could run amuck. Recent and useful discussions include: Novick (1988); Appleby, Hunt and Jacob (1995: Ch. 7); Evans (1997: Ch. 8); Haskell (1999: esp. Ch. 6). Two obvious conclusions emerge: the meaning of 'objectivity' is moot and the debate is old. We did not need the linguistic turn to learn that subjectivity is a standard historiographical problem: as Kitson Clark (1967: 10, 33) observed back in the 1960s, 'bias of one sort or another is protean and pervasive'. Historians hostile to the linguistic/postmodern turn are neither 'so naive' – nor so crassly positivist – 'as to deny that scholars inescapably bring discrete perspectives and values to bear upon their writings and lectures' (Fox-Genovese, 1999: xxi). The more interesting and important question is what we do about bias/subjectivity.

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first is to assume that, because complete objectivity is not possible, objectivity is in itself a misguided goal. This, as the economist Bob Solow once remarked, is like saying that, since a completely aseptic environment cannot be achieved, one might as well conduct surgery in a sewer.[9] Objectivity, like virtue, salvation, or nirvana, is something we should strive for, even if we know it is in fact beyond our reach. Getting closer is what matters.[10]

A second misconception is that being right – being faithful to what happened – is simply a question of factual accuracy. Getting particular facts 'right' – e.g. dating the outbreak of the Mexican Revolution to 1910 and not 1920 – is important, but it is only the start. As A. E. Housman put it, 'accuracy is a duty, not a virtue'.[11] It is only a start because huge edifices of error can be constructed on the basis of factually correct foundations. For example, during the First World War a slew of distinguished historians justified US belligerence in impeccably professional fashion – with facts duly footnoted, complete with all the scholarly apparatus; but the case they made – since they were engaged in a propagandistic form of historiography – did not withstand close scrutiny, to such an extent that, after the war, many of the contributors 'spent most of their subsequent vacations, sabbatical leaves and savings going round second-hand bookshops, furtively buying and burning all the copies they could find of their war-time utterances' (Novick, 1988: 127, quoting Herbert Heaton). All this happened because historians, caught up in the enthusiasm of the cause and persuaded, for once, of their own practical and patriotic usefulness, found that they 'could be flagrantly propagandistic without violating norms of scholarship narrowly conceived' (Novick, 1988: 126).

This example, apart from pointing up the dangers of politically partisan history (to which I will return), also demonstrates that 'getting it right' is a matter of balance and interpretation, not factual accuracy narrowly conceived. The conclusion is relevant to Mexican history, since many of the grand questions – concerning, for example, both the independence movement and the Revolution – hinge upon issues of typicality. If we are to generalise about independence or the Revolution, we need to discern and justify some kinds of pattern within the process. It is not enough just to fasten on random, arresting incidents or examples and it is quite wrong to elevate such incidents and examples to modal patterns, which supposedly explain the big picture. (In this context, note that 'the big picture' is relative.) If we try to paint a national portrait – as some naive fools

9 I cannot locate the source of the Solow observation. A similar point is made, in the more prolix parable style, by Hexter (1971: 103–106); and also by Fischer (1970: 42) n. 4: 'relativism mistakenly argues that because all historical accounts must be partial in the sense of incomplete, that they must also be partial in the sense of false'.

10 'It is not physics or nothing', as Moses Finley put it, rhetorically asking, 'is a limited reliability no better than useless?' (Finley, 2000 [1975]: 74). The same point is made, regarding historical narrative, by Appleby, Hunt and Jacob (1995: 235), where the authors proceed to argue for a position of 'practical realism' (247–51), which seems close to Finley's 'limited reliability', and no less sensible.

11 Cited in Carr (1990 [1961]: 10). However, while certain popular texts and potboilers may abound with errors (Ferguson, 2004, contains four errors on p. 59 alone), factual accuracy is not usually the principal problem when we discuss more serious academic history. There are, it is true, a few notorious cases where it rears its head (e.g. Evans, 1997: 116–23), but I know of no recent work on Mexican history where basic factual accuracy is a problem.

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do – then the experiences of particular states or regions become fragments. But state or regional histories also contain their – local or municipal – fragments; and even localities and *municipios* may encompass quite diverse patterns of behaviour. The *agraristas* of San José de Gracia were not typical of their community; Jean Meyer's Cristero veterans were not representative of all Jalisco or the centre-west (cf. Craig, 1983; González, 1983: Ch. 6; Meyer, 1973–1974; Purnell, 1999). In other words, typicality, context, and judgement all count; and, by neglecting these considerations, a work of history can be seriously flawed even when it is ponderously loaded with footnotes and presumably 'correct' archival citations. A history of the Mexican Revolution written on the basis of American insurance claims hardly seems a promising prospectus, for rather obvious reasons; but it has been done and has been warmly received (Hart, 2002).

(ii) Being *clear* means expressing historical narratives or analyses in ways that are lucid, accessible and consistent. This criterion, like the others, applies to all genres of history – hence, narrative and analysis (or, if you prefer, 'traditional' and 'scientific' history) alike.[12] It requires that style should, at the very least, serve the interests of clarity. This by no means rules out metaphor or other tropes; on the contrary, if well-chosen and well-handled, such figures of speech may contribute to clarity and understanding (in the same way that scientific metaphors do: waves, particles, superstrings, etc.).[13] Nor does it rule out jargon (that is, technical or abstruse terminology) which, as I have mentioned elsewhere, can promote clarity and speed of communication, especially where matters requiring particular expertise are involved (e.g. economic history, legal history, the natural sciences, American football) (Knight, 2002: 148–149). However, clarity *does* rule out gratuitous jargon as well as the weary repetition of hollow clichés and the mindless mixing of metaphors. Unfortunately, the linguistic turn in history does not seem to have enhanced linguistic precision and, as other critics have pointed out, some – not all – of the new cultural history is littered with these linguistic

12 As I have suggested elsewhere (Knight, 2002: 155), this narrative/analytical, or traditional/scientific, dichotomy is far too sharply drawn by some (e.g. Haber, 1999: 310–311, which follows Fogel and Elton, 1983). While they may constitute different styles or 'rhetorics', their underlying methodology is – or should be – much the same. Both Finley (2000: 69) and Peter Gay (1975: 189) make the same point.

13 'Literary devices are not separate from historical truth but the precise means of conveying it' (if used well, we might add) (Gay, 1975: 216); the writings of Ranke, who pioneered scientific history, are 'suffused with metaphor' (Evans, 1997: 22). Solow (1988: 34) (again) notes that 'the practice of economics is full of metaphors' (whose use and utility he then discusses). In the hardest of the hard sciences (physics), '"lines" of magnetic force (and) "bags" of quarks have been introduced because they are useful props in visualising what is going on' (Barrow, 2000: 18). Unfortunately, historians today sometimes seem oblivious to the mixing of metaphors and the mind-numbing effect it can have. Here's one random example, from a senior and influential scholar, which contrives to mix sonic, hydraulic and kinetic metaphors in one sentence and three lines: 'such *reverberations* yielded unplanned yet patterned convergences in the *flow* of scholarship rather than a unified intellectual *march*' (Stern, 2001: 37, emphasis added). My criticism may seem pedantic, but carelessness with words obscures argument and vitiates debate; sometimes, one suspects, it denotes a deeper failure to think through the argument being made.

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lapses and, as a result, it is quite often difficult to work out what its practitioners are trying to say (a harsh critic might suggest that this is because they are not sure themselves) (Socolow, 1999; note also Evans, 1997: 69–70, 200). These failings also jeopardise accessibility, perhaps even deliberately so. Accessibility is not, of course, a *sine qua non* of good history (still less of good economics or good astrophysics). Some history has to be erudite, technical, and aimed at a narrow specialist audience. But in general history is potentially more accessible than – to repeat my previous examples – economics or astrophysics, hence there should be a presumption than most history should be readily comprehensible to most historians and even most 'intelligent lay readers'.[14] 'I can conceive of no higher praise for a writer', Marc Bloch (1953: 3) wrote, 'than to be able to speak in the same tone to savants and schoolboys alike'. Perhaps that was a more realistic goal in Bloch's time than it is today; so historiography is not, in this regard, a record of progress.

(iii) Being *original* means saying something that has not been said before. A kind of pseudo-originality can be achieved by repackaging old goods in new labels; by calling widgets buggy-whip-receptacles or the like. Some historical sociologists have, like the friars of sixteenth-century Mexico, made their names by conducting such mass baptisms.[15] The most obvious and conventional way of achieving originality is to conduct primary-source research, thus to wrest new nuggets of information from the deep shafts of the archives (by which I mean 'archives' broadly defined; they may not always be documentary, but could include, for example, archaeological evidence and oral testimony). The 'advances' in Mexican historiography in the last 30 years (my approximate time-frame) have stemmed, more than anything else, from such primary research, for which there is no substitute (I give some examples later). Thus, historians who, having completed their doctoral-theses-turned-tenure-providing-monographs, then desert the archives and settle for synthetic work, tend to condemn themselves to a certain sterility. Marc Bloch (1953: 86) is again worth quoting: 'deprived of that perpetual renewal, that constantly reborn surprise, which only the struggle with documents can supply', such historians may 'lapse into a ceaseless oscillation between stereotyped themes imposed by routine' – or, we might add, by the RAE and comparable 'publish-or-perish' regimes.

Of course, synthetic work, based on secondary sources, can be valuable: by way of educating a broader public or, more usefully but more rarely, by presenting novel arguments which depend on an original and ingenious organisation of secondary-source data, perhaps inspired by cross-national comparison or broader social-science theory (for example, the work of Eric Wolf – both *Peasant Wars*, 1969; and *Europe and the People Without History*, 1983). But originality of this kind is hard to achieve. It demands a good grasp of both the particular empirical data and the grander theory

14 'Readily comprehensible' does not mean 'readily appreciable'. Thus, plenty of historiography, while devoid of arcane jargon, may demand specialist knowledge if its virtues (and faults) are to be properly appreciated.

15 I am thinking of the likes of Michael Mann and Anthony Giddens (prior to his brief incarnation as the organic intellectual of New Labour). For example: Mann (1986) and Giddens (1985). I would contrast these efforts with the superior historical sociology (or sociological history) of Eric Wolf (1969, 1983).

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or comparisons. Historians, by choice, temperament and training, are usually better at the first, hence their excursions into theory risk being – or seeming – simplistic and derivative (Womack, 2003). As I shall argue in conclusion, this does not matter much when the theory or comparison is imported, like some Oriental spice, in order to liven up a particular case study: thus, for example, we can add a dash of Weber to studies of *caudillismo* or populism without presuming to have read Weber in the original or to be experts in German sociology.[16] So, too, dollops of Durkheim or garnishes of Gramsci may enliven stodgy empirical studies. It is, I think, a common experience to read a perfectly sound monograph in which there is a sprinkling of Foucault – indicated not only by reverential name-dropping in the introduction, but also by subsequent telltale references to bodies, inscriptions, and archaeology – all of which, while it may convey a certain flavour, and even hint at the author's membership of a particular culinary clique, does not fundamentally affect the nutritional value of the work (that value being derived primarily from the empirical data, mediated through any number of authorial perspectives, the Foucaultian (*sic*) being but one of many and probably not the most influential).[17] So, originality tends to correlate with archival input; and works – including historiographical reviews like this – which lack such input are, in some senses, parasitic. Some, indeed, are parasitic, prolix, and fairly pointless.

I

Given these criteria, what can be said about recent Mexican historiography? I shall go on to argue that any attempt to review the field and – even more difficult – to distinguish the good from the bad, to identify gaps and advocate future directions for research, risks being overweening and presumptuous. But before making that negative argument, let me hazard a brief positive opinion. (And I use 'opinion' – with its connotations of subjectivity and tentativeness – deliberately). If I had to nominate areas of 'progress' – areas in which recent historiography has been particularly good, original, and fruitful, I would plump for three, taken from different periods of (post-independence) Mexico: the political history of the early ('Bourbon') republic; the economic history of the Porfiriato; and the local/regional history of the Revolution (1910–1940), especially in respect of what might loosely be called state-society relations. A word about each.

16 I base this on my own work: Knight (1980: 41–45, 58; 1998: 231). In both cases, but especially the first, the Weberian perspective helped, although it was not crucial to the entire argument. Regarding the first case, I would now qualify that perspective.

17 Not the most influential because, it strikes me, Foucault is a fairly light-footed and inconsistent thinker (and one who was hostile to rigorous grand theories), thus it is not clear what he would positively contribute to any specific piece of history (his negative critique of other theories is a different matter, but we hardly required Foucault in order to criticise highly fallible structural Marxism or modernisation theory or linear 'Whig' views of history). Thus, Foucault, rather like the God of Catholic New Spain, is a relaxed kind of intellectual deity, who demands some formal ritual obeisance but not rigorous compliance with a code of moral conduct.

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(i) We have recently seen a revival of *political history* (broadly defined) covering the period *c.* 1821–1854: Guerra (1993); Guedea (1993); Annino (1995: Ch. 5, 6); Rodríguez O. (1998, 2005); Warren (2001); Fowler (1998); Stevens (1991); Guardino (1996, forthcoming). This research has shown that the transition to independence brought major political change, even at the grassroots; that popular political mobilisation was extensive (and included peasants, Indians, and Afro-Mexicans as well as city-dwellers); that heterodox political ideas circulated briskly; and that popular actors were not only agents but agents possessed of political – as well as socio-economic – ideas. Thus, contrary to older assumptions, the plebeians had ideas in their heads and the fall of the colony was, to put it bluntly, a genuine political revolution (even if socio-economic continuity broadly prevailed). The 'middle period' may work in socio-economic terms, but it does not make much sense politically. Images of cloddish Mexican plebeians who went about imagining that the king still ruled a generation after Independence need to be rejected, or at least seriously qualified.[18]

(ii) A second area of fruitful recent research is the *economic history of the Porfiriato*, now analysed with greater objectivity and sophistication than before: in Marichal and Ludlow (1998); Cárdenas (2003); Haber (1997); Gómez-Galvarriato (1998); Kuntz Ficker and Riguzzi (1996); Passananti (2001); and Washbrook (2005), among others. Here, fresh data have been combined with economic expertise and illuminating comparisons; however, the results are accessible and not, that I can see, gratuitously mystifying. Such studies take the Porfiriato on its own terms (not just as the bleak preamble to the Revolution) and thus present a rounded picture – without (in most cases) succumbing to the kind of romantic revisionism which has coloured some social and political studies of the Revolution and Porfiriato.[19]

(iii) Perhaps the biggest cluster is that of *regional studies of the Revolution*, in both its armed phase (roughly, 1910–20) and its institutional, state-building phase (1920–40). This cluster is large in part because it is (relatively) old: it dates back to the early regional studies and to the pioneering *microhistoria* of Luis González in the late 1960s (González, 1983 [1968]). It is also large because 'regional' studies encompass a range of genres: some are genuine attempts at a kind of *gestalt* socio-political history (the 1970s norm, which was showcased in symposia like *Caudillo and Peasant* (Brading, 1980) and *Estadistas, Caciques y Caudillos* (Martínez Assad, 1988); some are more focused studies which necessarily adopt a regional approach because that, alone, is manageable [thus: labour history by Crider (1996); Snodgrass (2003); women's history by Olcott (2000); Mitchell (2002) and Boylan (2000)]. Recent regional studies also show something of the cultural turn: Becker (1995) and Boyer (2003) on Michoacán,

18 Cf. Escalante Gonzalbo (1992), *Ciudadanos imaginarios*: a stimulating but fallible book, written by a nonhistorian unacquainted with the archives and unfamiliar with recent historiographical trends. On the specific point: there may well have been some time-capsule communities or individuals who were unaware of the fall of the monarchy; but they were hardly as numerous or typical as Escalante – much influenced by the lamentations of nineteenth-century Mexican elites – chooses to believe; again, therefore, the question of what is typical or representative becomes key.

19 And which forms part of a broader rehabilitation of Díaz and the Porfiriato, evident even in the form of the telenovela (*Vuelo del águila*).

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Bantjes (1998) on Sonora, Vaughan (1997) on Puebla and Sonora, Fallaw (2001) on Yucatán, Brewster (2003) on the Sierra Norte de Puebla.[20] I suspect that this cycle of regional historiography is now drawing to an end: not because it is inherently spent (there are still plenty of un- or understudied regions, not to mention 'mute inglorious' localities, which await their 'revolutionary' historians; also, thematic histories – again, of labour, or of neglected themes like communications or consumerism – may well adopt regional approaches in order to achieve archival manageability); but rather because interests have shifted (the cultural turn is less enamoured of local and regional approaches) (e.g. Joseph, Rubinstein and Zolov, 2001); and because the chronological frontier has advanced, as it tends to do with the passage of time and the opening of archives.

Thus, future studies of this kind will, logically, break the 1940 barrier and penetrate into the historiographical no-man's-land of PRIísta Mexico, bringing the bright light of the historical gospel to dark places which have hitherto known only the baleful glow of PRI myth-making, or flickering beams of journalistic or political-science investigation. But since, for the time being, the main emphasis is more likely to be cultural, the old model of the *gestalt* sociopolitical study is probably on the wane, especially in the USA. Cooking, Cantinflas, comic books and rock music are the preferred themes; the socio-political foundations of the Pax PRIísta are, by and large, neglected.[21]

We might pause to ask why these trends occur. Of course, the causes of a historiographical trend do not determine its quality. If, for example, the Viet Nam war prompted studies of peasant insurrection (on both left and right, thus on the part both of those who sympathised with and those who sought to counter such insurrection),[22] it does not mean that those studies were inherently good or bad; like most subfields, this one contained both good and bad.[23] The Viet Nam example is one of external causality, derived from 'extra-academic' or 'extra-historiographical' factors, which may be contrasted with 'intra-academic' (sometimes also called 'cognitivist') factors: for example, the cross-pollination

20 A somewhat special category consists of studies of the Cristiada and Church-State relations in the revolutionary period: such studies are usually cultural and – not least because of the geographical limitations of the Cristiada – usually regional too. Apart from Meyer (1973–1974), Becker (1995) and Purnell (1999), we should include Butler (2004) and Serrano Alvarez (1992).

21 See Pilcher (1998, 2000); Rubinstein (1998); and Zolov (1999): all of them good and original books. I am merely observing, not criticising, the trend.

22 Hence the celebrated debate about peasant politics which pitted James C. Scott (1969) against Samuel Popkin (1979).

23 In general, it seems to me, we should refrain from judging the quality of any historiographical current or 'turn' on the basis of its 'externalist' provenance; the law of unintended consequences means that even unpromising 'turns' may yield positive results. There are, however, two partial exceptions to this rule of thumb: (i) where the turn is blatantly tendentious and antihistorical (for example, the 'Institute for Historical Review', which promotes holocaust-denial: see Evans (1997: 238–243); and (ii) where the turn becomes a senseless stampede, such that critical faculties are dulled and a great deal of dross is churned out. Even in the latter case, however, the dross should be identified on its own (lack of) merits, not inferred from its origins. Regarding historiography therefore we might take a leaf from the Code Napoléon: '*la récherche de la paternité est interdite*'. Carr (1990: 23), takes a contrasting view ('study the historian before you study the facts').

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of disciplines (which has produced 'Cliometrics' or 'anthrohistory') or the opening of archives, which prompts fresh research in new fields.[24] Regarding the three clusters just mentioned, we can probably hazard some guesses as to their origins. The political history of post-Independence partially obeyed an internal logic: the growing interest in political history which often stemmed from France and, in particular, from the dethronement of the 'social interpretation' of the French Revolution in favour of 'political culture' explanations by the likes of François Furet (1990) and Lynn Hunt (1984); which in turn reflected a broader shift from socio-economic and class explanations towards those rooted in ideas and culture. But external factors may also have played a part, given the resurgence of democracy in Latin America (including, in idiosyncratic form, Mexico) and the intellectual revalorisation of democracy which it entailed.[25]

Porfirian economic history carried the hallmark of Cliometrics (though the imprint was usually quite light), while also reflecting that rehabilitation of the Porfiriato as a period in its own right, which occurred *pari passu* with the delegitimation of the PRI and the myth of the Revolution. At the same time, the neo-liberal turn provoked interest in Mexico's earlier experience of export-led growth during the Porfiriato; and, what is often crucial in such shifts, certain concomitant institutional changes (in this case the florescence of centres of research and teaching like ITAM and CIDE) bridged the gap between 'internal' ('cognitivist') trends and 'external' ('real world'?) processes.

Finally, regional studies of the Revolution, which are the oldest cluster, reflected a growing belief that the old myth of the monolithic (and usually benign) Revolution needed serious qualification and that Mexico, being 'many Mexicos', produced 'many Revolutions' (hence Benjamin and McNellie, 1984; Benjamin and Wasserman, 1990). Again, broad intellectual trends (towards 'bottom-up', subaltern, and 'new social' history) linked Mexico to European and US historiography, at least since the 1960s; while improved access to archives made detailed research of this kind more feasible and attractive.[26] But, again, the

24 When it comes to explaining historiographical dynamics, the external/internal distinction – under various labels – is common and useful: e.g. Novick (1988): 9; Stern (2001): 33, 42.

25 Thus, through the 1960s and 1970s a standard radical-leftist argument stated that Latin American liberal democracy was a 'bourgeois' institution, at best superficial and superstructural, at worst a mere sham. The experience of authoritarianism helped discredit this view, while the 'third wave' of democratisation in the 1980s provoked renewed interest in the dynamics – and, to some extent, the benefits – of democratic regimes. The tendency to dismiss previous representative regimes (including those which came in the wake of independence) therefore diminished. Though these trends were, logically, most apparent in political science (hence the huge democratisation-studies industry of the 1980s and 1990s), historiography was also affected. I would hazard the opinion that this tide has now crested and begun to recede.

26 It is illuminating to compare this wave of scholarship, with its bottom-up, regional and local approach, to the work of the preceding generation, exemplified by the massive *Historia Moderna de México*, edited by Daniel Cosio Villegas: a work which, especially in its 'political' volumes, focused on national elites – the great men of the Restored Republic and Porfiriato – and which, even in its social and economic volumes, relied heavily on government publications and newspapers rather than state or local archives. Of course, it could be argued that the spadework of the older generation made possible the 'advances' of the younger.

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delegitimation of the PRI prompted revisionism: 1968 is, of course, the salient date, but we should also note the series of provincial movements, such as *Navismo* in San Luis Potosí, which challenged PRIísta centralisation and encouraged a renewed interest in local and regional history. This process partly involved the incorporation of an old tradition of such history ('amateur' investigations – sometimes panegyrics – of the *patria chica*) into a burgeoning higher education system; a system, furthermore, in which provincial centres, like the Colegio de Michoacán, came to play a major role.

In each case, therefore, internal and external factors counted. In some respects Mexico mirrored global trends; in others, the impulse was endogenous. The principle '*como México no hay dos*' must therefore be admitted into the debate; and we should be careful of assimilating all historiography – Mexican, American, European – to a common global framework: we are not that globalised. An obvious example would be the vogue for the new cultural history, which is appreciably stronger in the USA than in Europe or, indeed, in Mexico or Latin America (Joseph, 2001a: 4). This suggests that conditions peculiar to the USA may be at work: perhaps, the current fashion for identity politics, coupled with the historic weakness of class – roughly, classical Marxist – explanations in US historiography.

I made the point that the three clusters should not be judged according to their provenance, as I have briefly described it (which is a complicated provenance and, perhaps, a contentious description). Even if Porfirian economic history is in some sense driven by the neo-liberal turn, and even if we dislike the neo-liberal turn, that is no reason for dismissing the second cluster on that account. (Personally, I think that slagging off 'neo-liberalism' nowadays is a bit like berating 'Communism' during the Cold War – it is way too broad and indiscriminate, lumping where it should split and, in the process, creating a crude reification which certainly does not further our understanding of history). As in other cases, we should unpack each cluster and see what we make of it. Unpacking at once reveals, too, that clusters are not at all homogeneous. The first cluster contains Guerra, who tended to see Mexican plebeians as being locked into holistic 'old regime' communities, resistant to the new cerebral currents of liberalism and jacobinism, and Guardino, who shows how plebeians in both Guerrero and Oaxaca responded to these currents, assimilated new ideas and associational forms, and swiftly began to press their interests in these new terms. Within cluster three we find quite disparate interpretations of state-society relations in the revolutionary era (compare, say, Becker and Vaughan or, to a lesser degree, Vaughan and Bantjes). So, a cluster does not imply like-mindedness: indeed, it would be remarkable – and rather worrying – if it did.

Rather, a cluster is like a swirling galaxy, comprising a range of different components (white dwarfs, red giants – perhaps a black hole at the centre); it is held together by the gravitational pull of a common 'problematic' which, like most – perhaps all? – historical problematics, is linked to a specific period of history: thus, the ways in which politics operated in the new post 1821 environment; processes of economic 'development' in the Porfiriato; state-society relations in the revolutionary

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decades.[27] Interest in a common period or problematic does not entail likemindedness. Of course, economic historians have to trade quantitative data; but they do not necessarily agree about those data.[28] We can agree that the interface between 'state' and 'society' was a key feature of revolutionary Mexico without agreeing as to the nature of either state or society. Hence the lively recent debates about 'top-down' and 'bottom-up' processes of state formation in post-1920 Mexico. While these debates sometimes hinge upon contested concepts (state, civil society, hegemony) as well as contested 'facts', they are not endlessly circular and inconclusive: on the contrary, it now seems clear, for example, that the 'Leviathan' state presided over by President Cárdenas was much less of a Leviathan than previously supposed.[29]

II

This brings me to my penultimate discussion, which offers a critique of historiographical *prescription*, that is, the practice of laying down directions whither history should go. Pursuing my galactic metaphor (I will get back to cars in conclusion), I think we can roughly discern some existing galaxies (although there are no doubt many that elude us); we can identify some of their components; and we can hazard very tentative guesses about galaxy formation (the internalist and externalist causality mentioned earlier). But our vision is very limited and we fool ourselves if we attribute greater cohesion and homogeneity to galaxies than they in fact possess. We are also guilty of hubris if we think we can predict – or, in Godlike fashion, prescribe – the galaxies of the future. Unfortunately, historiographical resumés of this kind tempt the incautious historian to pontificate about where history has gone, is going, and both will and ought to go. That temptation should be resisted. Take, as a recent example, the stimulating edited volume *Reclaiming the Political in Latin American History* (Joseph, 2001b). This book is not specifically about Mexico, of course; but three of the five general 'think-pieces' (as opposed to case studies) have been written by distinguished

27 We could here compare historiography with political science, which tends to be much less rooted in specific times and places. Thus, while there may be shared interests (e.g. democratisation), the approaches will differ. Conventionally, historians are usually meant to use primary sources, which political scientists aren't; historians are likely to eschew the present and roam into the more remote past; and historiography is reckoned to be idiographic (particularist), political science nomothetic (in search of laws or broad probabilistic generalisations). All these – standard but debatable – differences reflect the fact that historical work is usually more convergent, political science more divergent; even when political scientists eschew ambitious cross-national comparison in favour of single case-study research, the goal is likely to be divergent (to use case study x to test hypotheses y and z). It is worth adding, however, that some recent Mexican political science has become much more historically (and sometimes anthropologically) focused and sophisticated: for example, Rubin (1997).

28 Thus, Kuntz Ficker and Riguzzi (1996) dissent from the founding father of Porfirian railway history, John H. Coatsworth (1981).

29 Bantjes (1998) and Fallaw (2001) offer valuable case studies from opposite ends of Mexico.

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Mexicanists (Joseph, Stern and Mallon) and they encapsulate some of the ideas which that powerful troika have eloquently expressed elsewhere.[30] Furthermore, these ideas touch on the nature of the discipline, the dynamics of Latin American (perhaps especially Mexican) historiography, and the direction that historiography might – and should – take in the future. So there is a threefold justification for considering *Reclaiming the Political* in this context: (a) it embodies some powerful analysis and advocacy by influential historians of Mexico (though not just Mexico) and, in doing so, it captures some of the pending issues in current historiography; (b) it is well known and has been robustly debated;[31] and (c) it offers a finite focus – it is something one can scrutinise with some degree of care and, if you like, textual precision; so, instead of plucking ideas and examples out of the air (no doubt erring and reifying as one does), one can get to grips with – indeed, one can 'deconstruct' – a particular printed text.

When it comes to 'reclaiming the political', the book appears to both describe and advocate; it has both an empirical and a normative dimension. The political is already being reclaimed – some reclamation is under way – but more needs to be done ('the political needs to be "reclaimed"') (Joseph, 2001a: 12); and the authors tell us how it might and should be done. This combination of description and advocacy, of 'is' and 'ought', is somewhat confusing; and the confusion (which may well be in the mind of the naive reader rather than the sophisticated author) is compounded by the dual meaning of 'reclaiming the political' which, as one reads and re-reads the relevant chapters, becomes rather clearer: on the one hand, we are talking about political historiography (thus the way that politics, broadly defined, is dealt with, and should be dealt with, by historians); but on the other hand we are also talking about politics in the world at large – a world in the 'viselike grip of neo-liberalism' (Joseph, 2001a: 3). So, in the first instance, it is a question (both empirical and normative) of how history is to be written and, in the second, of how historians are to engage in politics beyond the academy. Thus, just as we previously identified internalist and externalist explanations – causal inputs, to put it mechanistically – of historiographical trends over time, so we now encounter both internalist (academic, historiographical, 'cognitivist') and externalist (local, national, global) outputs: the contributions that historians are making and should be making in order, supposedly, to 'reclaim the political'.

Of the two, the call to 'reclaim the political' within history seems rather otiose, since it has never been left out. In fact, both Joseph and Stern recognise that, their book title notwithstanding, political history has never been overlooked ('there has never been an abandonment of the political'; 'politics has never been underemphasised in the Latin American context' (Joseph, 2001a: 4). At most, the meaning of 'political' has shifted – as, indeed, has the meaning of quite a lot of things; and, they conclude, they are not

30 For example, Mallon (1994: 1491–1515); Joseph (1998); Stern (1993: 3–20). In lumping these three together I am following their own counsel: as students of Emilia Viotti da Costa, we are told, they belong to 'a somewhat distinctive discursive community within the profession' and they further represent 'incipient trends of hemispheric convergence in the study of Latin America's past' (Joseph, 2001a: 9).

31 See Womack's review (2003) and Stern's reply (2003).

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really reclaiming but rather 'revisiting' or 'remaking' the political (Joseph, 2001a: 5; Stern, 2001: 32).

Not so long ago, we might recall, some political scientists made a similar appeal – to '*Bring the State Back In*' (Evans, Rueschmeyer and Skocpol, 1985).[32] But their recommendations, especially in the context of American political science, made a certain amount of sense, since they were seeking to counter classic Marxist and (*a fortiori*) liberal-pluralist views, which demoted the state (thus, much of the political) to secondary status, either as a mere reflex of class (hence, the 'agent state', the so-called 'executive committee of the ruling class') or as a neutral pluralist arena where interest groups jostled and jockeyed in peaceful competition for resources. But their call was hardly relevant to history, where the state and 'the political' had never been left out (witness my clusters, especially one and three, not to mention a host of earlier studies). And we should certainly not take political scientists, especially American political scientists, as our methodological mentors; or we might as well chuck away our passports and desert the archives in favour of computer consoles where, in the comfort of our air-conditioned offices, we can consume and crunch statistical data bases all day.

The parallel is worth mentioning because one apparent target of the 'reclaiming the political' mission is old-style Marxist history: specifically, 'monochromatic explanations based on class and imperialism' (Joseph, 2001a: 11). Now, I like polychromatic history as much as the next person; but I don't think the monochromatic version was ever so extensive or influential as is suggested here. US historians (of Latin America or anywhere else) have been disproportionately non or anti-Marxist (Novick, 1988; Appleby, Hunt and Jacob, 1995: 80–81); if there was a 'monochromatic' view of Latin America, based on simplistic notions of class or imperialism, it emanated not from historians but sociologists and a few renegade political scientists.[33]

Perhaps (and here I'm guessing) the call to 'reclaim' – or to 'revisit' or 'remake' – the political constitutes a critique of the new cultural history, even though that is a genre which these authors have favoured and creatively contributed to. The new cultural history, Joseph states, 'defangs and expunges' the political (Joseph, 2001a: 3). Even if that's true (and I am frankly agnostic about it, since I can't claim to know all the relevant work and I suspect that 'political' is often used to mean different things by different people) (compare Vaughan, 1999), it is not clear to me what is being proposed by way of remedy. Stern laments that 'so many potentially inspiring narratives' have come to an end and that there has been a 'collapse of paradigms of inspiration' (2001: 49). [Some of these paradigms, of course, were themselves of rather monochromatic Marxist hue; indeed, Stern seems to include the defeat/collapse of the Soviet Union in this bleak picture of paradigmatic change (2001: 49)]. That there has been – that we

32 Evans, Rueschmeyer and Skocpol (1985). I should forewarn the reader that the critique of American political science which follows does not apply to the contributors to this book, *Bringing the State Back In*, who – the Latin Americanists certainly – are far removed from the stereotype which I proceed to criticise.

33 Such as James Petras, Maurice Zeitlin, Ronald Chilcote, and (with caveats) Andre Gunder Frank.

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are living amid – such a 'paradigm shift' is stated more or less explicitly in the book: Stephen Haber is taken to task, *inter alia*, for 'his lack of engagement of (*sic*) the history of scientific thought and philosophy'; since, we are told, ever since Kuhn, 'the ability to "see and respond" to the unexpected has become a far more complex historical problem than that acknowledged in Haber's modelling of valid epistemology and method' (Stern, 2001: 47].

What is Thomas Kuhn doing here? Have we historians been living through a kind of Copernican revolution in historiography without knowing it?[34] If so, what are its revelatory findings? What was the previous Ptolemaic system that now lies in pieces? Joseph makes the point that contemporary historiography is fragmented, which is probably true (although the comment sits a little uneasily next to Tom Skidmore's identification of a generation of so-called 'integrators') (Joseph, 2001a: 3, 10, citing Skidmore, 1998). But when wasn't historiography fragmented (or, to put it the other way round, when was there a single hegemonic bloc in the academy)? We may live in times of uncertainty and upheaval, but when did we not? (Of course, it partly depends on who 'we' is; in this discussion 'we' usually refers to US historians and, quite often, to radical or progressive US historians). Compared to the 'fragmented' 1990s and the 2000s, were, say, the 1960s and 1970s any more cosy and consensual (notwithstanding Viet Nam, the draft, Governor Reagan, the Kent State killings)? The McCarthyite 1950s perhaps? The years of the Second World War and incipient Cold War? The Depression? The decade of the First World War and the ensuing Red Scare?[35] Was there ever a time when consensus ruled and historians were of like mind?, and would it have been a good thing if it had and they were? There is an oddly unhistorical quality about these laments for the good old days. A feature of today's 'intellectual hard times' is, we are told, 'the lack of a cohesive social movement in the United States' (Joseph, 2001a: 12). As Talking Heads used to sing: 'same as it ever was, same as it ever was'.

With these questions we find ourselves leaving the leafy groves of academe and entering the 'real world' outside. The broader message of the book – that historians of Latin America should 'reclaim the political' by means of their engagement in that world (and not simply through their scholarship) – is made clearly and consistently. Emilia Viotti da Costa, we are told (Joseph, 2001a: 12):

> constantly reminded her students of the obvious: that history changes; that cycles of despair and hope are inextricably bound up with that change; and that what is important about methods for understanding the past is not their cleverness or their effectiveness in debunking what came before, but their usefulness in helping us engage politically with the world.

34 There is a fairly obvious paradox here. If we are unaware of it, a Copernican Revolution can hardly have occurred (unless 'we' denotes a handful of Ptolemaic reactionaries too stupid to see what has happened all around us). Regarding the utility and relevance of Kuhn, see note 55 below.

35 Novick (1988) gives recurrent examples of dissent and disputes (for example, between 'relativists' and their opponents) throughout the twentieth century. Note also Appleby, Hunt and Jacob (1995: 158).

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This is a remarkably radical statement. Some would also say a radically positivist statement.[36] And it could be taken as a dismissal, or a demotion, of colonial history (or medieval history, or any history whose links to the present are more stretched and tenuous).[37] But it is a variation on a fairly familiar theme: in the present mess, 'what is a progressive scholar to do?' (Mallon, 1994: 1491). Whatever that question means, and whatever answer it elicits,[38] it is clearly a call to action, addressed to historians (or, an important qualification, a subgroup of 'progressive' historians) and it is a call for them to deploy their history in the service of broader progressive political goals. These goals

36 'No one today, I believe, would dare to say, with the orthodox positivists, that the value of a line of research is to be measured by its ability to promote action'. Which ivory-tower waffler wrote that? Marc Bloch (1953: 9), who, of course, was a veteran of the First World War, a 53 years old volunteer in 1939, a Jew who, soon after he wrote these lines in 1941, became active in the Resistance and, in 1944, was captured, tortured and shot by the Germans. It is interesting to note that, today, political relevance is more often associated with historical relativism (Fox-Genovese, 1999: xiv).

37 It will be objected that the political relevance of history does not neatly correlate with temporal proximity; ancient events (1789, 1688, 1492) can have powerful contemporary resonance. Croce put this view in extreme form: 'all history is contemporary history' (Evans, 1997: 30). However, it is not really like this. There is a rough (not neat) correlation between the recency and the relevance of historical events, if only because the causal chains are shorter. If we really want to know why the PRI lost in 2000, we are better off studying the election of 1994 than that of 1910. Furthermore, the relevance of distant events may have little to do with serious historical research. The political impact of the 1992 Quincentenary was not created by cutting-edge historical research; on the contrary, 1992 tended to encourage simple essentialist views of Indians which went against the grain of recent scholarship. History usually becomes politically relevant because of present-day pressures (social, political, economic); and political actors usually pick and choose the history they want to promote (it is not usually difficult for them to find historians who will oblige). President Salinas's doctrine of 'social liberalism' pandered to the ambitious Salinista project and played fast-and-loose with nineteenth-century Mexican history; it would be nonsense to pretend that Ponciano Arriaga suddenly acquired political cachet, *c.* 1990, because fresh research had rescued him from the musty archives and presented him to a grateful and pleasantly surprised president.

38 Stern (2001: 49) tells us that this now well-known opening line – 'what is a progressive scholar to do?' – is meant to be ironic (and he should know). I understand that irony is intellectually chic these days, but this explanation makes little sense. Irony involves saying the opposite of what is meant. Ironic interrogatives are, for this reason, odd, unless the interrogative is a rhetorical question strongly implying a given answer, like those signalled in Latin by *num* and *nonne* (thus, 'who could possibly doubt the PRI's profound commitment to democracy?' would be half-baked PRIísta rhetoric if delivered at face value – expecting the answer 'no one' – but it would be ironic, and more plausible, if asked by a critic of the PRI). Mallon's interrogative is rhetorical, and she proceeds to try to answer it; but the reader, while probably anticipating that an answer is coming, has no idea what that answer will be. Such indeterminacy is no basis for irony. Again therefore a supposed trope leads to further confusion. As the Fowlers (1931: 25) presciently observed: 'the word irony is one of worst abused in the language'. Stern gets it wrong again on p. 63, n. 29, where an alleged error of Haber is deemed 'deeply ironic'. You can't have unwitting irony; Stern means 'self-contradictory'.

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may be estimable and we may agree with them. We may vote, march, campaign and contribute accordingly. But should we research, write and teach accordingly? (Note that the question, if put to a chemist or physicist, would be rather bizarre).[39] In other words, should we bend our history to these goals? Or, to put the question another way, should we restrain our history when it may seem to run counter to them? Thus, should we refuse to touch Pinochet as a subject for historical investigation because, Stern says, he is a 'master architect of "radical evil"' for whom 'the dash of sympathy required for profound historical understanding proves impossible, pointless or perverse'?[40] Are we strangely resuscitating the old adage, *tout comprendre, c'est tout pardonner* (minus the *tout*, of course, which no one believes in nowadays)?

This, the broad question of the political consequences of historiography, is no mere 'academic' question. It is the question which David Stoll presumably debated (with himself) before producing his critique of Rigoberta Menchu (Mallon, 2001: 329–30, 352). It is also the question which US historians had to confront when dealing with the First World War, as mentioned earlier. And it is a standard question in historical theory: although in that context the question is often framed by relativists who – rebutting their 'absolutist' opponents – describe the political engagement of historians not as an exalted calling to be pursued but as an inevitable fact of life to be regretted. Indeed, in that context the 'noble dream' is not political engagement but historical objectivity,[41] and that's why Joseph's claim is remarkably radical.

So, we face two related but distinct questions. First, within the narrower bounds of historiography, are the diagnosis and remedy proposed by *Reclaiming the Political* persuasive? And, briefly reprising a much older debate, should we, as historians, make a virtue of political engagement? More specifically, should we, as Mexicanists, use our work, say, to sustain progressive social movements or to counter the revival of Roberto Madrazo's PRI?

Regarding the first (historiographical) point, I would enter several caveats. It is hard enough to determine – retrospectively – where historiography has gone over time. Stern's generational synopsis is useful and sometime apposite; but there are several

39 I do not mean that it would be bizarre to query the application of science, thus to make political judgements about, say, nuclear energy or genetic engineering. Those judgements might even involve abstaining from research which is seen as potentially dangerous. It would, however, be bizarre to bring political criteria to bear in the process of researching (or teaching) nuclear fission or the human genome. Remember Lysenko; and, more pertinent to the contemporary United States, the Scopes monkey trial.

40 Stern (2001): 55. Would this approach not eviscerate holocaust studies? In my own book about the Mexican Revolution (Knight, 1986: Ch. 1, esp. 91–103) I spent a good deal of time trying to fathom the character and regime of Victoriano Huerta, a ruthless and sanguinary military *golpista*. It did not occur to me to classify him among the ranks of the 'radically evil', still less to draw a veil of silence over his actions. Was that a symptom of blunted 'sensibilities' on my part? Evans (1997: 51) goes further: 'a historian who uses terms like "wicked" and "evil" about a person or persons in the past will only succeed in looking ridiculous'.

41 'Absolutist' could also read 'objectivist' or 'realist'. See Novick (1988); Carr (1990); Evans (1997: Ch. 8).

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problems, not least a good deal of slippage between schools of thought and scholarly generations.[42] We have also seen that generalisations about both current trends (e.g. the alleged apolitical character of the New Cultural History) and previous academic scenarios are open to serious question. So, even retrospective (empirical) diagnosis is difficult.

It is difficult for two main reasons. First, conceptually, it is far from easy to define, say, 'political', 'social' and 'cultural' history (not to mention relativism, positivism, objectivism, pragmatism and all the other isms which are said to inhabit the spacious mansion of Clio). Thus, if we are not careful, we start trading overblown reifications, which is sometimes the first step towards a narrow sectarianism. As I argued regarding my 'clusters', what is important is not the reified label which one applies, but the common problem(s) being addressed and the quality of the answers being given.

Second, I doubt that anyone can command the field – a field that expands by the day – to the extent of making confident generalisations about past and present trends, let alone confident predictions and prescriptions about the future.[43] It is like overflying the Mexico City rush hour in a helicopter trying to direct the traffic with a megaphone. Prescription makes more sense to the extent that it is very limited and focused, thus confined to areas which the prescriber knows pretty well and, I would further stress, to the extent that it is free from generic – not to mention sectarian – categorisation. Thus, within the third cluster, which I know about, I would suggest that we might benefit from new studies of the CROM in the 1920s, of the outcome of the Revolution in the state of Morelos in the 1920s and 1930s, of urban (as opposed to rural) schooling, and of the internal mechanisms of the PNR, especially as it mutated into the PRM in 1938. I say this because these topics have been relatively neglected; the questions are important; and, by filling these lacunae, we might be able to establish

42 The identification and dating of successive historiographical schools/generations in Stern's (2001: 37) chapter are, as he himself concedes, 'too neat', and there are historians – like the 'conceptually precocious' Steins (59 n. 7) – who don't quite fit the sequence. There is also a recurrent tendency to exaggerate historiographical breakthroughs by presenting an excessively bland two-dimensional backdrop: thus, 'great man history' prevailed in Europe until the *Annales* school came along (37); both Mallon and Joseph are credited with 'counterintuitive' findings (regarding peasant nationalism in nineteenth-century Peru and popular mobilisation in revolutionary Yucatán, respectively, 44, 46), both of which judgements depend on what was 'intuitively' believed before. (I don't think any historian argued that Yucatán was a hotbed of revolutionary mobilisation).

43 This begs the question of what 'the field' is. In this paper I have ranged over Mexican history in the national period; however, I would not pretend to be a paid-up expert on the nineteenth century; and, like most historians, my knowledge of post-1950 Mexico is somewhat superficial. So I would feel genuinely confident only when discussing my third cluster. Stern (2001) strives to cover all Latin American history: he makes a brave attempt, but it is compromised by the daunting mismatch between the mountains of data and the limitations of any one mind, however fast it reads, digests, and evaluates. 'Every student ought to know that mastery is acquired by resolved limitation', as Lord Acton (1975: 18) put it (and he was not afraid of the occasional heroic generalisation). The problem is not, of course, confined to Mexico or Latin America; it is probably even more acute in European and US history (Evans, 1997: 173).

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some useful connections to both earlier and later processes (thus, to use an ugly and overworked term, there would be a certain synergy involved). I would not, however, wish to legislate that such studies – of the CROM, Morelos, schools, or the party – should be in the mode political, the mode social, the mode socio-political, politico-social, socio-cultural, politico-cultural or any other convoluted genre that you care to name. The important thing is that they are done and done well by good historians. Thus, to my way of thinking, to say: Mexican history in general needs more politics or less culture (or vice versa) is too stratospheric and, almost by definition, too subjective, being necessarily based on inadequate information.

In this respect, history is not much different from other disciplines (or even human activities in general). In all major fields of inquiry – including the social sciences and, from what I know, the natural sciences – broad prescription from on high is very likely to fail. This is not only because the field is vast, but also because the means of predicting and prescribing, thus of 'picking winners', are totally inadequate. The same is true of financial analysts, market pundits, economists, and businessmen. IBM, General Electric and RCA all turned down the photocopier; Lord Nuffield, offered Volkswagen's Wolfsburg plant at the end of the war, also declined. Market analysts are mostly charlatans; 'chartists', in particular, are no better than astrologers. The British Government's 'foresight committees', set up to prescribe the course of hi-tech investment in the 1990s, have proven as myopic as the rest of us (Kay, 2004: 87–119).

Why should historians be any better?[44] At the risk of recycling a hoary cliché, it seems to me that history should be a market place. In these days of globalisation and neoliberalism, the ideology of the market has been noisily boosted, and just as noisily denounced. In fact, what passes for the 'market' (the market of Enron, Halliburton, the European CAP, the business career of George W. Bush) is far removed from the free market of competing individuals analysed by the original classical economists; and that is the kind of market I am talking about – a market of canny individuals looking for their niche and trading on their wits; not a market of massive corporations, vested interests and allied government bureaucracies. (Note that academia has its more modest counterparts: the foundations and endowments which may adopt highly prescriptive policies regarding research. Their prescription tends, certainly in the UK, towards a narrow utilitarianism; the ESRC is not trying to foment political revolution. But the pointlessness of prescription is much the same, whatever the ulterior goals).

At the level of particular institutions and historical subgroups, prescription again is risky. Here the danger is that of cloning, whereby mentors produce students in their

44 Stern (2001: 52) does modestly concede that 'one cannot program research passions or achieve clairvoyance', but then – 'with some trepidation' – he broaches 'the necessary twists and turns that lie ahead', which is where prescription ('one part prediction and four parts advocacy') comes in (53–57). Though I know of no systematic study, I suspect that historians' predictions are as bad as anyone else's (see, for example, Evans, 1997: 59–62); the contrasting comment of Appleby, Hunt and Jacob (1995: 10) – that 'what historians do best is to make connections with the past in order to illuminate the problems of the present and the potential of the future' – suggests that they have encountered a different (and much more prescient) species of historian than the one I know.

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own image. (Oddly this seems to be more pronounced in the USA, a nation of supposed rugged individualism. I admit that this is no more than a speculation, based on impressionistic evidence and hearsay; but it might be susceptible to empirical testing).[45] Of course, mentoring may fail (it may even prove counter-productive); and, in some cases, it may reflect a logical and efficient form of self-selection – when it comes to doctoral work, for example, young economic historians may gravitate towards Stanford, cultural historians to Yale or Wisconsin, labour historians to Harvard or Duke, old fogeys with private incomes to Oxford, and so on.[46]

However, clarion calls to reorient the discipline go far beyond this narrow, logical, system of apprenticeship; they more resemble declarations of missionary fervour. Convert to Cultural History and Ye shall be saved! Outside the Church of Cliometrics there is No Salvation! (I exaggerate slightly for the sake of effect).[47] Sometimes, of course, the calls to repentance are more reasonable, muted and middle-of-the-road (following the style of the Anglican Church rather than Pentecostal revivalism): what we want is a 'healthy' synthesis of agency and structure;[48] *pas d'extrême*, a bit of this, a bit of that; what we might call the beatifics of *bricolage*.[49] But, leaving aside questions of clarity and consistency,[50] who says that *bricolage* is best? Is the middle of the road the right place to be? Maybe explanations of, say, 'class in the last analysis' are sometimes right. Maybe the flight to the centre is sometimes wrong.[51] Again, it depends on the time, the place, and the problem.

These caveats concern academic history (and its internal politics). But, as we have seen, there is prescription which goes beyond the narrow confines of historiography and seeks to engage with the world outside. This, too, strikes me as rather lofty and

45 It is also an old and recurrent allegation: Novick (1988: 181); Kors (1999: 16).

46 To spare the blushes of my Oxford students I should clarify that this is tongue-in-cheek (maybe even ironic).

47 The over-selling of a particular historiographical line is hardly new: as Bloch (1953: 150) put it over 60 years ago, 'science dissects reality only in order to observe it better by virtue of converging searchlights whose beams continually intermingle and interpenetrate each other. Danger threatens only when each searchlight operator claims to see everything by himself, when each canton of learning pretends to national sovereignty'. It is worth recalling how, at regular intervals, dedicated followers of historiographical fashion have stressed the superiority of their own style – for example, with social history in the 1960s and 1970s: Evans (1997): 168–169.

48 Stern (2001: 41; or, again, 52): 'a healthy future lies in a rather eclectic building of complementary paths to politically meaningful, socially inclusive and sensitive understandings of the history of the oppressed'. Amen to that.

49 *Bricolage*: ad hoc improvisation (see Novick, 1988: 133, following Lévi-Strauss).

50 The careful reader of both Joseph (2001a) and (*a fortiori*) Stern (2001) becomes aware that strong prescriptions (do this, avoid that) accompany much looser suggestions (hang loose, tune in, turn on), the latter serving to qualify, even subvert, the former; thus, at times, the reader feels as if he is shuttling between a seminary and a saloon bar. I have tended to focus on the prescriptive arguments, which at least have the merit of being more forceful, clear-cut, and falsifiable.

51 'There are in the world scholars whose good nature has worn itself out in seeking a middle ground between antagonistic statements' (Bloch, 1953: 112).

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quite likely counter-productive. As Stern rightly says, historians should not exaggerate their own importance; we should beware of 'overblown appraisals of the politics of scholarship' (2001: 33). I would tend to go further: historians usually acquire immediate 'real world' influence insofar as they serve the interests of power-holders (governments, business, churches). In the UK, and perhaps the USA, it is conservative historians who have most successfully deployed history in the service of current politics (and to the advantage of their own careers). That's not because they are better historians producing better history, but because their messages chime in with certain current realities and receive generous support and airtime. Of course, if the left espouses instrumentally progressive history, it can hardly complain when the right beats it at its own game. Indeed, this may be a game the left can't win, so should not get involved in. After all, if the world is such a mess, a scholarly and disinterested analysis of the mess and how we got into it might be the most creatively subversive contribution that historians can make.

Second, the resort to prescription, preaching, and political-flagwaving will frighten the horses. That is to say, if the task is to win over opinion, it is presumably undecided – or even hostile – opinion which counts. Preaching should be directed at the pagans, not the devout Christians. Yet the rhetoric of prescriptive history, with its heavily normative terminology ('cycles of despair and hope', 'treacherous and often heart-wrenching terrain', 'agonizing denouement', 'bittersweet meditation') is hardly calculated to appeal to a broad audience (Joseph, 2001a: 6, 12; Stern, 2001: 49, 51). (It even puts off some on the left, it seems). Compare, as a single contrasting example, the passage from Viotti da Costa's *Crowns of Glory, Tears of Blood*, describing the death of the rebel leader Quamina, cited, in *Reclaiming the Political*, by Daniel James: a passage which, he says, 'draws its power from restraint, from the sparseness of its literary devices', in which 'the narrator maintains her distance, refusing to explicitly evoke outrage, pity or sympathy' (James, 2001: 362).

Third and finally, the logic of political prescription is likely to result in more dull, predictable and unpersuasive history. It implies that we should massage our research; that some topics are off-limits (e.g. Pinochet?); and that some are to be treated with kid-gloves (e.g. Rigoberta Menchu?). Among other things, there is a whiff of paternalism here, redolent of Sartre's '*il ne faut pas desespérer Billancourt*' (that is, we leftist intellectuals should shut up about Stalin for fear of disillusioning the Communist workers in the Billancourt Renault factory).[52] In short, this approach assumes a superior wisdom and expressly legitimises 'externalist' inputs. When it comes to writing history, such inputs are to a degree inevitable, as we all know. Complete detachment, absolute objectivity, are unattainable. But should we celebrate and replicate such external inputs, or, rather, regard them, like the 'flu virus, as facts of life we have to live with?

52 It seems that Sartre never quite said this; a character in his 1956 play *Nekrassov* says something roughly similar; and the mud stuck. But the phrase usefully captures a recognisable attitude.

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III

Finally, a change of tropes: from astronomy to automobiles. Among the great cultural gifts of the USA to the world – the Bill of Rights, jazz, talking pictures – one that should not be disdained is Car Talk, a radio programme in which two very smart guys, the Tappet brothers, diagnose callers' car problems. We, as historians, are like the Tappet brothers (although we are probably not as smart as them). By and large, we learn on the job and deal with specific problems.The kind of problems we can usefully and constructively tackle are (to take some recent – and fairly weighty – examples, drawn from my clusters): how did the people of Oaxaca react to and participate in the new politics of post-Independence? What were the economic rationale and impact of Porfirian railroads? Why were Federal schools more successful on the left bank of the Yaqui River than in the Sierra Norte de Puebla? Answers to these weighty – but manageable – questions, if they are convincing (that is, well-evidenced) and clear (thus, lucidly and consistently organised), are also likely to be original (since the questions remain open ones); so the end result is good history, according to the three criteria I sketched earlier. It matters not a jot whether the answers are social, economic, political, cultural, top-down, bottom-up, nuanced, nested, structural, or contingent (whatever these often vague labels might mean). It may matter, in practical 'real-world' terms, whether the answers give aid and comfort to wicked conservatives or gratify the 'sensibilities' of progressives;[53] but to take such 'externalist' considerations into account *ex ante* – that is, to place history at the service of politics, seems to be misguided, patronising, and, in the long run, probably counter-productive anyway. It will not do the cause much good.[54]

The questions listed above – Oaxaca, railroads, schools – are specific and bounded (though, of course, they may form part of much grander questions concerning political legitimacy or economic development). They therefore resemble Car Talk questions: why does my '87 Ford Mustang stall at the lights? What is the insistent knocking in the

53 By my rough count, 'sensibility' (singular and plural) appears 25 times in Stern (2001). There are six appearances on p. 34 alone. Judging by context, it can mean: 'attitude', 'taste', 'opinion', 'persuasion', 'concern', 'concept', 'mentality', and 'perspective'. We get 'a sensibility that informs research', 'political sensibility', 'a Gramscian sensibility', 'the twin sensibilities of agency and structure', 'sensibilities about class', 'new sensibilities', 'sensibilities of perception' 'a mixed sensibility' and 'our sensibilities of authenticity' (this last one flummoxed me completely). Sense and sensibility, which Jane Austen felicitously joined together, here seem to have been cruelly put asunder.

54 If the historian feels strongly that certain research will prove politically damaging, he or she can always refrain – from publishing, writing up, or even starting the research in the first place. Stoll had those options. But discreet silence is a sad resort, since it assumes that 'good' causes depend on self-censorship. Milton's advice – 'let truth and falsehood grapple: whoever knew truth put to the worst in a free and open encounter?' – may seem overly optimistic, but it contains a basic and robust truth. As I said earlier, if things are so bad, uninhibited original research might at least help explain why they are, while exploding prevailing myths and misconceptions. And the research will be more credible if it remains discreetly scholarly and does not shout its political credentials from the rooftops.

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back (is it the rear differential or that Mafia snitch we loaded earlier)? Historical questions (and answers) like Car Talk questions (and answers) occupy a hierarchy. Some are minute and mundane (hence the recondite titles of scholarly articles which become objects of fun, often unfairly). These are the equivalent of changing the spark plugs. Near the top of the hierarchy are entire engine replacements – that is, major works which substantially alter the field (Womack on Zapata and Katz on Villa would be two examples with which I am familiar. Incidentally, it would be misconceived to talk even of these as 'Kuhnian' paradigm shifts, but then it is misconceived to talk of such shifts in almost all contexts, scientific as well as historiographical).[55] A good deal of the history we do or read falls somewhere in between. To go beyond – to overview whole swathes of history, while making general prescriptive recommendations – is to undertake a quite different and quite probably pointless activity. It is like trying to design a hydrogen-powered car, or building a nuclear fusion engine for installation in the next Hyundai Accent. It is very difficult to do (indeed, it may be impossible) and we are not really trained to do it. Nuclear fusion is for particle physicists, not car mechanics. As historians we should stick to changing the spark plugs, occasionally re-boring the cylinder block, maybe, if we are ambitious, installing an entire engine. These tasks can be tricky enough in themselves, but at least they are useful (and if the job goes wrong, we can usually see why). The rest – the hydrogen engine, the nuclear fusion car – are best left to others (such as philosophers of history); and, at most, we should speculate about them just occasionally, before we get back to the real job with the oily rags in the repair shop.

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55 Kuhn's views shifted over time; the more moderate (1974) version is humdrum, the more radical (1962) version is wrong, except perhaps in the classic case of the Copernican revolution, which is what Kuhn (a historian) really knew about. Kuhn's views are not held in particularly high regard by natural scientists; they do not explain post-Copernican scientific advance; and they are of little use for historians, since (to my knowledge) there has never been a paradigmatic shift in historiography (see n. 34), which rather tends to advance incrementally and which permits the peaceful coexistence of numerous different approaches (none of which are 'paradigms' in the robust Kuhnian sense). As far as I understand it, science is not that different in its *modus operandi*. I do not fully understand Stern's discussion (2001: 64, n. 30) of Kuhn (for example: what are 'unifying laws of logical determination of matter'?), but it seems to me to inflate and/or misconceive the impact of both Kuhn and Einstein. See Barrow (2000: 374–377) and Kitcher (1993: 128–132, 173–177, 272–276, 294).

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